February 20, 1951

Dr. Roger Y. Stanier
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Dear Roger,

This letter is primarily to let you know of certain developments which have arisen since your visit here. You may recall that I told you the day before you left that we intended to try the UV system in yeast. Well, the method worked very beautifully indeed and we have been using it ever since as a means for studying kinetics. It is far superior in many respects to the system we had previously employed which used arsenate to stop adaptation at various levels. Its most useful feature resides in the fact that UV unlike arsenate or azide, does not interfere, apparently, with other synthetic activities. Thus one can separate experimentally the formation of the apoenzyme from the formation of any special intermediates required in the expression of the activity of the apoenzyme formed. This is particularly valuable in the case of maltose and galactose utilization. As apparently you had already observed in pseudo-monas, UV does not interfere with assimilatory activity. I might add by the way that we get the same kind of curves with the UV system as we had already obtained with arsenate.

One other development: you may recall we were not, when you were here, completely certain about the existence of a specific -phenyl-glucosidase. We have, however, since succeeded in separating this enzyme out free of any maltose or - methyl splitting capacity. So, it would thus appear that there is another distinct and separate enzyme towards a synthetic substrate. There is no doubt, however, that the maltase also has -phenyl splitting capacity.

I got around to reading the Horowitz review and I must confess I was not too overly impressed with it, which probably does not surprise you. I could not resist writing him a note in which I raised a few issues with him, in particular his discussion of the implications, or rather lack of them, of the kinetics of adaptation. I told him that I thought that his note of warning about being cautious of interpretations based upon homogeneous reaction kinetics was a noteworthy one. However, with respect to the autocatalytic kinetic interpretation of adaptation, I had to confess to him that I was not completely certain as to what we were supposed to be cautious about. The derivation of the autocatalytic kinetics

as is, for example, involved in establishing the logistic equation, contains no assumptions or restrictions which limits the applicability of that equation to homogeneous reaction systems. Indeed, as I could not resist pointing out to him, one would hardly consider a herd of elephants or a population of E. coli a homogeneous reaction system; nevertheless, the logistic equation quite adequately describes, under certain conditions, the law of growth. I am anxiously awaiting his reply. I actually do not consider such criticism as valid and I think indeed that it is based on a misunderstanding of what was being attempted and also on a misunderstanding of the basic assumptions involved in such derivations.

Please give my fond regards to Mike and Rita. There is a good chance that Helen and I will be able to get out to the west coase at the end of May. I have a panel meeting out there, at present scheduled for May 23, and I am hoping we can get up there a little bit before and pay you folks a visit. As soon as things really crystalize with respect to that, I will let you and Mike know about it. In the meantime, tell Mike to hurry with his prospective program and to be sure to submit it through our panel. I don't think he will have any difficulty in getting the kind of support that he requires. We have, as I say, this meeting in May coming up which should give him ample opportunity to get things ready; but try to impress upon him the necessity of not waiting until the last minute.

Sincerely yours,

S. Spiegelman

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